NOTES BY THE EDITOR.

ESPY AND THE FRANKLIN KITE CLUB.

In response to a letter from the Editor inquiring about the history of the Franklin Kite Club, Dr. Wahl, Secretary of the Franklin Institute, has been making some inquiries through the columns of the Philadelphia Ledger, and although his investigation is not yet complete, yet some facts have been elicited which are of general interest in view of the certainty that hereafter the kite will be an important accessory in meteorological explorations of the atmosphere.

The Franklin Kite Club is referred to by Prof. James P. Espy in his Philosophy of Storms, published in 1841. It is there stated, page 167, that a report was made by the Franklin Kite Club in which they announce the discovery that on those days when columnar clouds formed rapidly and numerously their kite was frequently carried upward nearly perpendicularly by columns of ascending air, and that this circumstance became so familiar during the course of their experiments that on the approach of a columnar cloud, just forming, they could predict whether it would come near enough to affect their kite, for if the cloud did not pass directly over the kite, the latter would only move sideways toward the cloud. This paragraph was apparently written in the winter of 1837–38, and the report of the club probably referred to the year 1837.

Even before this time Espy seems to have flown his own kite in order to test his formula for calculating the height of a cloud, for on page 75, in a paragraph that was written before September, 1834, he says:

Since writing the above a kite was sent up into the base of a cloud, and its height ascertained by the sextant and compared with the height calculated from the dew-point, allowing 100 yards for every degree that the dew-point was below the temperature of the air; and the agreement of the two methods was within the limits of the errors of observation. In this case the base of the cloud was over 1,200 yards high. Moreover the motions of the kite, whenever a forming cloud came nearly over it, proved an up-moving column of air under it. I speak of cumulus clouds in the form of sugar loaves with flat bases.

The following items relative to the history of the Franklin Kite Club are extracted from the Philadelphia Ledger of October 6, 1896.

The Ledger has made diligent search for some record of the Club, and has learned that such a club did, undoubtedly, exist; but whether as an organized body or not can not yet be determined. It was contemporary with the Archery Club, which flourished about 1840. So far as can be learned the kiteflying was conducted simply for recreation. but from the well-known character of the people who took part in the sport it is more than likely that there were some among them who took a scientific interest in the matter, and who would have reported any discoveries they might have made.

From Charles J. Hayes, the well-known landscape engineer, it is learned that Franklin Peale was a prominent member of the Kiteflying Club, and had a large box in which the kites were kept, which box Mr. Hayes now has in his possession. The kites have long since disappeared. The club had regular times for the sport, and used to go out on the hills at Fifteenth and Green streets, and to Pratt's Gardens, where the Lincoln Monument now stands in Fairmount Park. Mr. Hayes was a member of the Archery Club, which also frequented the same grassy fields for its sport, and on many occasions saw the kites raised, and often took part in the sport. They were mostly imported from China, and were made in fantastic shapes and were gaudily painted. The "man kites" represented men in flowing coats and with rolling eyes. They would be flown so as to hover over vessels on the Schuylkill, exciting terror or mirth in the spectators or occupants of the boats. Snakes and dragons were also among the forms assumed by these toys. The box of kites finally found a resting place at the residence of Mr. Baldwin, of locomotive fame, at Wissinoming, where the club used to go from time to time to fly them. Peale owned them. Mr. Hayes had no knowledge of any scientific use being made of the sport.

Mr. Horace Sellers says that his uncle, George Escol Sellers, remembers the interest taken in kiteflying by the members of the Archery Club, but has no recollection of any special organization. According to his recollection Franklin Peale, Baldwin Keyser, Samuel Griffiths, and Harvey L. Sellers were among the members of the club who took part in the kiteflying experiments. He remembers hearing of a sled being rigged up one winter so as to be drawn by kites when there was unusually good skating on the Delaware. He also refers to kites made by Franklin and Linnæus Peale at their father's home in Germantown. He recalls one large kite, 5 feet by 3 feet, covered with tough hardware paper, which was controlled by a reel swivelled at the top of a post back of the barn. He says that one day they placed a kitten in a basket attached to a parachute hung to the tail of this kite, and by means of lighted Chinese punk arranged to burn the string after a certain interval. The basket was made to fall, and the parachute opening before reaching the ground the kitten was safely landed.

This recollection of Mr. Sellers would go to show that scientific experiments and sport were combined when the club went out for exercise on pleasant afternoons. If it could be established that the club experimented in meteorological matters it would now have a great historical and scientific interest, in view of the present experiments of the Government Weather Bureau in the employment of kites for the study of atmospheric phenomena.

ISOBARS AND THEIR ACCURACY.

The charts published in the MONTHLY WEATHER REVIEW present monthly and annual and sometimes normal isobars for the years and months. These lines are irregular curves, and in comparing them with the monthly resultant wind direction, one is often forced to ask himself how reliable are the minuter details in these curves, and to what extent must the lines be literally followed in order to get a correct idea of the relation between the pressure and the wind. In a general way we know that the wind blows around and in toward the center of a region of low pressure, and we are rather surprised to find that what is true in the stormy portions of a daily weather map is not also universally true on the maps of monthly isobars and winds. Thus if we follow along the isobar of 30.00 for 1895, as shown on Chart I of the summary for that year, we find seven cases where the wind arrows blow from the higher toward the lower side of this line, and also seven where the wind arrows blow from the lower toward the higher side. Several of these exceptional cases occur in the Dakotas, where the 30.00 line has a double curvature, making two loops or bights, and similar loops are found in other isobars on this map, as well as in the isobars drawn by Mr. Morrill and published on Chart IV of the same issue.

The reliability of isobars must depend, primarily, on that of the barometers in use at the station, secondarily on the method of reduction to sea level, and finally on the judgment of the student who draws the lines as published. The sources of uncertainty and the extent to which each may affect the result are about as follows:

1. Uncorrected errors within the instrument, such as the so-called zero of the scale, the capillarity, the error of the attached thermometer, the difference between the temperature shown by the attached thermometer and the real temperatures of the mercury and the scale. The sum total of all these, if they should accidentally happen to work in the same direction, might amount to several hundredths of an inch but they generally counterbalance each other and the outstanding average effect for the month is supposed not to exceed plus or minus 0.01.

2. The use or the neglect of the correction for the variation of local gravity from its standard or average value at sea level and 45° of latitude. This correction for gravity is, properly speaking, an instrumental correction, it is peculiar to the mercurial barometer and does not apply to the aneroid barometer. This correction is frequently neglected so that most of the published isobars show apparent and not standard pressures. The extent to which the neglect of this correction affects the isobars depends upon the latitude of the

station, the altitude of the barometer above sea level and the existing pressure. So far as latitude is concerned its influence for 30 inches of mercury is given by the figures printed on the right hand side of each map of isobars and the correction varies from plus 0.027 inch at latitude 55°, to minus 0.061 inch at latitude 20°. The correction has its full value, just given, at any station when the pressure is 30 inches and increases or diminishes exactly in proportion as the pressure departs therefrom; it has, therefore, two-thirds of its value when the pressure is 20 inches and it has one-thirtieth more than its full value when the pressure is 31 inches. As concerns altitude, the correction increases algebraically but slowly with altitude. If the force of gravity were determined at every station, as it easily could be, a somewhat more correct value of the influence of this source of error would be known.

3. The isobars depend upon pressures that "have been reduced to a sea level." This reduction is made for the purpose of, at least approximately, annulling the influence of the height of the station, thereby making the reduced pressures more nearly comparable than are the actual station pressures. This would be unnecessary if the stations were all at the same altitude, and it becomes a very uncertain hypothetical quantity when the stations vary in altitude from sea level up to 7,000 feet, as is the case in the present American system. Other things being equal, the uncertainty of the reduction to sea level must increase with altitude. For the averages of a month or a year the methods or systems of reduction used by conservative students will differ 0.10 inch for an elevation of 5,000 feet, and 0.01 for an elevation of 1,000 feet.

4. Besides the uncertainty due to methods, the reduction to sea level is also uncertain because of unknown errors in the adopted elevations of the barometers at the stations. The greater part of the elevations used by the Weather Bureau depend upon levelings made with spirit levels by railroad and canal engineers. Many discrepancies occur among these levels; very few of our altitudes are considered reliable to within 10 feet, and an uncertainty of 20 feet in the elevation, or 0.02 inches in the reduced barometer is considered a fair index to the accuracy of the elevation in the interior of the country; in a few special cases it far exceeds this.

5. In view of the preceding it is not proper to publish reduced observations to a greater degree of refinement than the nearest 0.01 of an inch, although the preliminary calculations are all made to the nearest thousandth, consequently the published figures even at the low stations have an uncertainty of plus or minus 0.005 due to the adoption of the nearest whole hundredth, whereas they might agree to within one or two thousandths if the third decimal had been retained on the charts.

In view of the preceding the student who has charted his pressures reduced to sea level and is about to draw the isobars for every 0.05 of an inch of pressure may well ask how closely his pencil must follow the figures that his eye interpolates between the charted numbers. As he wishes to show the narrow belt over which the pressure is 30.05 and as his pencil traces accurately the 30.05 line in among the maze of figures a little larger or smaller than this, he is perpetually oppressed by the conviction that a little wavering to the right or left can do no harm because the figures are slightly uncertain. When he finally has traced upon his map several loops or bights, such as those shown in the annual maps for 1896, there is a strong temptation to wipe them out and replace these by a smoother generalized line; a temptation that is intensified when he perceives that the winds, which, according to our preconceived ideas ought to have a simple relation to these isobars now appear to be entirely independent of them.

Now the well-recognized proper way of presenting any data that results from observation is to follow the numerical results strictly, but to accompany them with some indication of their probable reliability. Thus in the present case the isobars represent closely a rigorous interpolation between the charted numbers, but the lines thus interpolated have different degrees of reliability depending upon the several sources of uncertainty above enumerated: this uncertainty can be best represented on a chart by a shaded area extending equally on either side of the isobar to a distance representing the supposed uncertainty of 0, 1, 2, 3, etc., hundredths of an inch. Such shading would probably not look well on the published maps, but can easily be supplied by any special student. When the shaded areas approach each other closely, or overlap, then we know that the reduced pressures within the shaded region are too uncertain to justify reliance upon the isobar drawn within it. In such cases we must seek to combine the pressures at several neighboring stations into one normal that shall be more reliable than any one of the individual pressures. By using several such normals isobars may be drawn that shall give some idea of the distribution of pressure at sea level. In respect to the bights and loops on the annual chart in the MONTHLY WEATHER REVIEW for 1895, the shaded areas do sometimes overlap in the Rocky Mountain Region, where the isobars on Chart I show dotted lines, but the bights in the Missouri Valley, on Charts I and IV, still remain, although they might plausibly be made less pronounced. We conclude, therefore, that in the study of isobars and isotherms, as in the study of every other matter that results from observation, one must always present the figures correctly, but guard against drawing conclusions finer than are warranted by the reliability of the data.

THE FIRST ATTEMPT TO MEASURE WIND FORCE.

Meteorological observers, especially those who have studied the development of anemometry, will recall the fact that the most simple and direct measurement of the velocity of the wind is made by observing the speed of light bodies, such as feathers or soap bubbles carried along by it. The first piece of apparatus applied to the measurement of the wind was the pendulous plate anemometer introduced by the Royal Society about 1665 on the recommendation of Sir Christopher Wren, Robert Hooke, and others, who constituted a committee on meteorological observations. This instrument gave a measurement of the effect of moving air on a resisting plate from which the velocity can perhaps be calculated. In using this and almost all other apparatus which measures some definite effect of the wind it is assumed that the wind blows upon the apparatus long enough to bring its moving parts into a steady condition, either of motion or of resistance, so that we measure the maximum effect that a given wind is capable of producing. Prof. C. F. Marvin has called our attention to the fact that meteorology owes another ingenious method to Sir Isaac Newton. This eminent philosopher was for many years engrossed in the study of forces; he it was who first saw that the proper method of measuring and comparing forces among themselves is to measure the amount of energy that each force when acting continuously can communicate in a unit of time to a unit mass of freely moving matter. It seems to have occurred to him to apply this idea to the resistance of the wind. When a body is falling freely through the air the resisting force of the air sometimes erroneously called friction is brought into play; this resistance can be expressed by the amount of retardation experienced by a falling body whose mass and resisting area are, respectively, unity. Newton also applied this same idea to an ingenious method of determining the relative strength of the various winds. His experiments in this line are narrated at page 15 of Sir David Brewster's Memoir of the Life, Writings, and Discoveries of Sir Isaac Newton, whence we take the following: